Philosophical Transactions

Please note: Due to an error in the print volume, the page numbering in this article may contain either page numbering skips, or page numbering repetitions, or both. However, the article content is presented in its entirety and in correct reading order.

Please click on "Next Page" (at the top of the screen) to begin viewing the article.

I. *A Letter to the Right honourable* George *Earl of* Macclesfield *concerning an* apparent Motion *obſerved in ſome of the* fixed Stars; *by* James Bradley *D. D. Aſtronomer Royal, and F. R. S.*

My LORD,

Read at a Meeting of the Royal Society, Febr. 14. 1747.

THE great Exactneſs, with which Inſtruments are now conſtructed, hath enabled the Aſtronomers of the preſent Age to diſcover ſeveral Changes in the Poſitions of the heavenly Bodies; which, by reaſon of their *Smallneſs*, had eſcaped the Notice of their Predeceſſors. And altho' the Cauſes of ſuch Motions have always ſubſiſted, yet Philoſophers had not ſo fully conſider'd, what the Effects of thoſe known Cauſes would be, as to demonſtrate *a priori* the *Phænomena* they might produce; ſo that Theory itſelf is here, as well as in many other Caſes, indebted to Practice, for the Diſcovery of ſome of its moſt elegant Deductions. This points out to us the great Advantage of cultivating *this*, as well as every other Branch of Natural Knowledge, by a regular Series of Obſervations and Experiments.

The Progreſs of Aſtronomy indeed has always been found, to have ſo great a Dependence upon accurate Obſervations, that, till ſuch were made, it advanced but ſlowly: For the firſt conſiderable Improvements that it received, in point of Theory, were owing to the renowned *Tycho Brahè*; who far exceeding thoſe that had gone before him, in the Exactneſs of his Obſervations, enabled the ſagacious *Kepler* to find out ſome of the principal Laws, relating to the Motion of the heavenly Bodies. The Invention of Teleſcopes and Pendulum-Clocks affording proper Means of ſtill farther improving the *Praxis* of Aſtronomy; and theſe being alſo ſoon ſucceeded by the wonderful Diſcoveries made by our Great *Newton*, as to its Theory; the Science, in both reſpects, had acquired ſuch extraordinary Advancement, that future Ages ſeemed to have little room left, for making any great Improvements. But, in fact, we find the Caſe to be very different; for, as we advance in the means of making more nice Inquiries, new Points generally offer themſelves, that demand our Attention. The Subject of my preſent Letter to your Lordſhip, is a Proof of the Truth of this Remark: for, as ſoon as I had diſcovered the Cauſe, and ſettled the Laws of the Aberrations of the fixed Stars, ariſing from the Motion of Light, *&c.* whereof I gave an Account in N°. 406. of the *Philoſophical Tranſactions*; my Attention was again excited by another *new Phænomenon*, *viz.* an annual Change of Declination in ſome of the fixed Stars; which appeared to be ſenſibly *greater* about that time, than a Preceſſion of the Equinoctial Points of 50″ in a Year would have occaſioned. The Quantity of the Difference, tho' ſmall in itſelf, was rendered perceptible, thro' the Exactneſs of my Inſtrument, even in the firſt Year of my Obſervations; but being then at a Loſs to gueſs, from what Cauſe that greater Change of Declination proceeded, I endeavoured to allow for it in my Computations, by making uſe of the *obſerved* annual Difference, as mentioned in *p.* 652. of the ſame *Tranſaction.*

From *that* time to the preſent, I have continued to make Obſervations at *Wanſted*, as Opportunity offered, with a View of diſcovering the Laws and Cauſe of this *Phænomenon:* For, by the Favour of my very kind and worthy Friend *Matthew Wymondeſold* Eſq, my Inſtrument has remained, where it was firſt erected; ſo that I have been able, without any Interruption, which the Removal of it to another Place would have occaſioned, to proceed on with my intended Series of Obſervations, for the Space of twenty Years: a Term ſomewhat exceeding the whole Period of the Changes, that happen in this *Phænomenon.*

When I ſhall mention the *ſmall* Quantity of the Deviation, which the Stars are ſubject to, from the Cauſe that I have been ſo long ſearching after; I am apprehenſive, that I may incur the Cenſure of ſome Perſons, for having ſpent ſo much Time in the Purſuit of ſuch a ſeeming Trifle: But the candid Lovers of Science will, I hope, make due Allowance for that natural Ardour, with which the Mind is urged on towards the Diſcovery of Truths, in themſelves perhaps of *ſmall* Moment, were it not that they tend to illuſtrate others of greater Uſe.

The apparent Motions of the heavenly Bodies are ſo complicated, and affected by ſuch a Variety of

Cauſes; that in many Caſes it is extremely difficult to aſſign to each its due Share of Influence; or diſtinctly to point out, what Part of the Motion is the Effect of one Cauſe, and what of another: And whilſt the joint Effects of *All* are only attended to, great Irregularities and ſeeming Inconſiſtencies frequently occur; whereas, when we are able to allot to each particular Cauſe its proper Effect, Harmony and Uniformity uſually enſue.

Such ſeeming Irregularities being alſo blended with the unavoidable Errors, which Aſtronomical Obſervations muſt be always liable to, as well from the Imperfection of our Senſes, as of the Inſtruments that we make uſe of, have often very much perplex'd thoſe, who have attempted to ſolve the *Phænomena*: and till Means are diſcovered, whereby we can ſeparate and diſtinguiſh the *particular* Part of the whole Motion, that is owing to each reſpective Cauſe, it will be impoſſible, to be well aſſured of the Truth of any Solution. For theſe Reaſons, we generally find, that the more exact the Inſtruments are, that we make uſe of, and the more regular the Series of Obſervations is, that we take; the ſooner we are enabled to diſcover the Cauſe of any new *Phænomenon*. For when we can be well aſſured of the Limits, wherein the Errors of the Obſervations are contain'd; and have reduced them within as narrow Bounds as poſſible, by the Perfection of the Inſtruments which we employ; we need not heſitate to aſcribe ſuch apparent Changes, as manifeſtly exceed thoſe Limits, to ſome other Cauſes. Upon theſe Accounts it is incumbent upon the *practical*

Aſtronomer, to ſet out at firſt with the Examination of the Correctneſs of his Inſtruments; and to be aſſured that they are ſufficiently exact for the Uſe he intends to make of them: or at leaſt he ſhould know, within what Limits their Errors are confined.

This Practice has, in an eminent manner, been lately recommended by your Lordſhip's noble Example; who having, out of a ſingular Regard for the Science of Aſtronomy, erected an Obſervatory, and furniſhed it with as complete an *Apparatus* of Inſtruments, as our beſt Artiſts could contrive; would not fully rely on their Exactneſs, till their Diviſions had undergone the ſtricteſt Re-examination: whereby they are probably now render'd as perfect in their kind, as any extant, or as human Skill can at preſent produce.

The Lovers of *this* Science in general, cannot but acknowledge their Obligations to your Lordſhip on this Account; but I find myſelf more particularly bound to do it; ſince, by means of your Lordſhip's moſt accurate Obſervations, I have been enabled to ſettle ſome *principal Elements*; which I could not at preſent otherwiſe have done, for want of an Inſtrument at the Royal Obſervatory, *proper* for that Purpoſe: For the large *mural Quadrant*, which is there fixed to obſerve Objects lying Southward of the Zenith, however *perfect* an Inſtrument it may be in it ſelf, is not *alone* ſufficient to determine, with proper Exactneſs, either the *Latitude* of the Obſervatory, or the Quantity of Refraction correſponding to different Altitudes: For it being too heavy to be conveniently removed; and the Room wherein it is placed, being too ſmall to admit of its being turned to the oppoſite Side of the Wall, whereon it now hangs; I cannot, by *actual* Obſervations of the circumpolar Stars, ſettle thoſe neceſſary Points; and therefore have endeavoured to do it, by comparing my own with your Lordſhip's Obſervations: and until this Defect in the *Apparatus* belonging to the Royal Obſervatory be removed, we muſt be indebted to your Lordſhip, for the Knowledge of its true Situation.

A Mind intent upon the Purſuit of any kind of Knowledge, will always be agreeably entertained, with what can ſupply the moſt proper means of attaining it: Such, to the practical Aſtronomer, are exact and well-contriv'd Inſtruments; And I reflect with Pleaſure on the Opportunities I have enjoyed, of cultivating an Acquaintance and Friendſhip with the Perſon, that, of all others, has moſt contributed to their Improvement. For I am ſenſible, that if my own Endeavours have, in any reſpect, been effectual to the Advancement of Aſtronomy; it has principally been owing to the Advice and Aſſiſtance given me by our worthy Member Mr. *George Graham*; whoſe great Skill and Judgment in Mechanicks, join'd with a complete and practical Knowledge of the Uſes of Aſtronomical Inſtruments, enable him to contrive and execute them in the moſt perfect manner.

The Gentlemen of the *Royal Academy* of *Sciences*, to whom we are ſo highly obliged for their exact Admeaſurement of the Quantity of a Degree under the Arctic Circle, have already given the World very convincing Proofs of *his* Care and Abilities in thoſe Reſpects; and the particular Delineation, which they have lately publiſhed, of the ſeveral Parts of the *Sector*, which he made for them, hath now rendered it needleſs, to enter upon any minute Deſcription of *mine* at *Wanſted*; both being conſtructed upon the ſame Principles, and differing in their component Parts, chiefly on account of the different Purpoſes, for which they were intended.

As mine was originally deſigned to take only the *Differences* of the Zenith Diſtances of Stars, in the various Seaſons of the Year, without any View of diſcovering their *true* Places; I had no Occaſion to know exactly, what Point on the Limb correſponded to the *true* Zenith: and therefore no Proviſion was made in my Sector, for the changing of its Situation for that Purpoſe. Neither was it neceſſary that the Diviſions or Points on the Arc ſhould be ſet off, with the utmoſt Accuracy, Equidiſtant from each other; becauſe, when I obſerve any particular Star, the ſame Spot or Point being firſt biſected by the Plumb-line, and then the Screw of the Micrometer turn'd until the Star appears upon the middle of the Wire, that is fixed in the common Focus of the Glaſſes of the Teleſcope; I can thereby collect, how far the Star is from that given Point at the Time of Obſervation: and afterwards, by comparing together the ſeveral Obſervations that are made of it, I am able to diſcover what apparent Change has happen'd. The Quantity of the viſible Alteration, in the Poſition of the Stars, being expreſſed by Revolutions and Parts of a Revolution, of the Screw of the Micrometer; I endeavoured to determine, with great Care, the true Angle anſwering thereto: and after various Trials, I thoroughly ſatisfied myſelf, both of the Equality of the Threads of the Screw, and of the preciſe Number of Seconds correſponding to them.

But altho' theſe Points could be ſettled with great Certainty, I was nevertheleſs obliged to make one Suppoſition; which perhaps to ſome Perſons may ſeem of too great Moment in the preſent Inquiry, to be admitted without an evident Proof from Facts and Experiments. For I *ſuppoſe*, that the Line of Collimation of my Teleſcope has invariably preſerved the ſame Direction, with reſpect to the Diviſions upon the Arc, during the whole Courſe of my Obſervations. And indeed it was on account of the Objections, which might have been raiſed againſt ſuch a *Poſtulate*, that I thought it neceſſary, to continue my Series of Obſervations for ſo many Years, before I publiſhed the Concluſions, which I ſhall at preſent endeavour to draw from them.

Whoever compares the Reſult of the ſeveral Trials, that have been made by the Gentlemen of the *Academy* of *Sciences*, for determining the Zenith Point of their Sector, ſince their Return from the North; will, I preſume, allow that *mine* is not an unreaſonable or precarious *Suppoſition:* ſince it is evident, from their Obſervations, that the Line of Collimation of that Inſtrument underwent no ſenſible Change in its Direction, during the Space of more than a whole Year; altho' it was ſeveral times taken down, and ſet up again in different and remote Places; whereas *mine* hath always remained ſuſpended in the ſame Place.

But beſides ſuch a ſtrong Argument for the Probability of the Truth of my *Suppoſition*, I have the Satisfaction of finding it *actually* verified by the

Obſervations themſelves; which plainly prove, that at the End of the full Period of the Deviations which I am going to mention, the Stars are found to have the ſame Poſitions by the Inſtrument, as they ought to *have*, ſuppoſing the Line of Collimation to have continued unaltered from the Time when I firſt began to obſerve.

I have already taken notice, in what manner this *Phænomenon* diſcover'd itſelf to me at the End of my firſt Year's Obſervations, *viz.* by a *greater* apparent Change of Declination in the Stars near the Equinoctial Colure, than could ariſe from a Preceſſion of 50″ in a Year; the mean Quantity now uſually allowed by Aſtronomers. But there appearing at the ſame time, an Effect of a quite contrary Nature, in ſome Stars near the Solſtitial Colure, which ſeem'd to alter their Declination *leſs* than a Preceſſion of 50″ required; I was thereby convinced, that all the *Phænomena*, in the different Stars, could not be accounted for, merely by ſuppoſing, that I had aſſumed a wrong Quantity for the Preceſſion of the Equinoctial Points.

At firſt, I had a Suſpicion, that ſome of theſe ſmall apparent Alterations in the Places of the Stars, might poſſibly be occaſioned by a Change, in the Materials, or in the Poſition of the Parts of my Sector: But, upon conſidering how firmly the Arc, on which the Diviſions or Points are made, is faſtened to the Plate, wherein the Wire is fixed that lies in the Focus of the Object-Glaſs; I ſaw no Reaſon to apprehend, that any Change could have happened in the Poſition of that Wire and thoſe Points. The Suſpenſion therefore of the Plummet being the moſt likely Cauſe, from whence I conceived any Uncertainty could ariſe; and the Wire of which had been broken three or four times in the firſt Year of my Obſervations: I attempted to examine, whether Part of the 'foremention'd apparent Motions might not have been owing, to the different Plumb-lines that had been made uſe of. In order to determine this, I adjuſted a particular Point of the Arc to the Plumb-line, with all the Exactneſs I could; and then taking off the old Wire, I immediately hung on another, with which the ſame Spot was again compared. I repeated the Experiment three or four times, and thereby fully ſatisfied myſelf, that no ſenſible Error could ariſe from the Uſe of different Plumb-lines; ſince the various Adjuſtments of the ſame Point agreed with each other, within leſs than half a Second.

Having then, from ſuch Trials, ſufficient Reaſon to conclude, that theſe *ſecond* unexpected Deviations of the Stars, were not owing to any Imperfection of my Inſtrument; after I had ſettled the Laws of the Aberrations ariſing from the Motion of Light, *&c.* I judged it proper to continue my Obſervations of the ſame Stars; hoping that, by a regular and longer Series of them, carried on thro' ſeveral ſucceeding Years, I might, at length, be enabled to diſcover the *real* Cauſe of ſuch apparent Inconſiſtencies.

As I reſided chiefly at *Wanſted*, after my Sector was erected there in the Year 1727. till the Beginning of *May* 1732. when I removed from thence to *Oxford*: I had, during my Abode at *Wanſted*, frequent Opportunities of repeating my Obſervations; and thereby diſcovered ſo many Particulars relating to theſe *Phænomena,* that I began to gueſs what was the real Cauſe of them.

It appeared from my Obſervations, that, during this Interval of Time, ſome of the Stars near the Solſtitial Colure, had changed their Declinations 9″ or 10″ *leſs,* than a Preceſſion of 50″ would have produced; and, at the ſame time, that, others near the Equinoctial Colure, had altered theirs about the ſame Quantity *more,* than a like Preceſſion would have occaſioned: the North Pole of the Equator ſeeming to have approached the Stars, which come to the Meridian with the Sun, about the Vernal Equinox and the Winter Solſtice; and to have receded from thoſe, which come to the Meridian with the Sun, about the Autumnal Equinox and the Summer Solſtice.

When I conſider'd theſe Circumſtances, and the Situation of the Aſcending Node of the Moon's Orbit, at the time when I firſt began my Obſervations; I ſuſpected, that the Moon's Action upon the Equatorial Parts of the Earth might produce theſe Effects: For, if the Preceſſion of the Equinox be, according to Sir *Iſaac Newton*'s Principles, cauſed by the Actions of the Sun and Moon upon thoſe Parts; the Plane of the Moon's Orbit being at *one* time, above ten Degrees *more* inclined to the Plane of the Equator, than at *another*; it was reaſonable to conclude, that the Part of the whole annual Preceſſion, which ariſes from her Action, would in different Years be varied in its Quantity; whereas the Plane of the Ecliptic, wherein the Sun appears, keeping always nearly the ſame Inclination to the Equator; *that* Part of the Preceſſion, which is owing to the Sun's Action, may be the ſame every

Year: And from hence it would follow, that, altho' the *mean* annual Preceſſion, proceeding from the joint Actions of the Sun and Moon, were $50''$; yet the *apparent* annual Preceſſion might ſometimes exceed, and ſometimes fall ſhort, of that mean Quantity, according to the various Situations of the Nodes of the Moon's Orbit.

In the Year 1727. when my Inſtrument was firſt ſet up, the Moon's Aſcending Node was near the Beginning of *Aries*; and conſequently, her Orbit was as much inclined to the Equator, as it can at any time be; and then the *apparent* annual Preceſſion was found, by my firſt Year's Obſervations, to be greater than the *mean:* which proved, that the Stars near the Equinoctial Colure, whoſe Declinations are moſt of all affected by the Preceſſion, had changed *theirs*, above a tenth Part more than a Preceſſion of $50''$ would have cauſed. The ſucceeding Years Obſervations proved the ſame Thing; and in three or four Years time the Difference became ſo conſiderable, as to leave no Room to ſuſpect, that it was owing to any Imperfection, either of the Inſtrument or Obſervations.

But ſome of the Stars, which I had obſerved, that were near the Solſtitial Colure, having appeared to move, during the ſame time, in a manner contrary to what they ought to have done, by an Increaſe in the Preceſſion; and the Deviations in them being as remarkable as in the others, I perceived that ſomething more, than a mere Change in the Quantity of the Preceſſion, would be requiſite to ſolve this Part of the *Phænomenon*. Upon comparing my Obſervations of Stars near the Solſtitial Colure, that were almoſt oppoſite to each other in Right Aſcenſion; I found, that they were equally affected by this Cauſe; for whilſt γ *Draconis* appeared to have moved Northward, the ſmall Star, which is the 35th *Camelopardali Hevel.* in the *Britiſh* Catalogue, ſeem'd to have gone as much towards the South: which ſhew'd, that this apparent Motion, in both thoſe Stars, might proceed from a Nutation in the Earth's Axis; whereas the Compariſon of my Obſervations of the ſame Stars, *formerly* enabled me to draw a different Concluſion, with reſpect to the Cauſe of the annual Aberrations ariſing from the Motion of Light. For the apparent Alteration in γ *Draconis*, from *that* Cauſe, being as great again as in the other ſmall Star, proved, that *that Phænomenon* did not proceed from a *Nutation* of the Earth's Axis; as, on the contrary, *this* may. Upon making the like Compariſon between the Obſervations of other Stars, that lie nearly oppoſite in Right Aſcenſion, whatever their Situations were with reſpect to the Cardinal Points of the Equator, it appeared, that their Change of Declination was nearly equal, but contrary; and ſuch as a Nutation or Motion of the Earth's Axis would effect.

The Moon's Aſcending Node being got back towards the Beginning of *Capricorn* in the Year 1732, the Stars near the Equinoctial Colure appeared, about that time, to change their Declinations no more, than a Preceſſion of $50''$ required; whilſt ſome of thoſe near the Solſtitial Colure altered *theirs* above $2''$ in a Year leſs, than they ought. Soon after, I perceived the annual Change of Declination of the former to be diminiſhed, ſo as to become *leſs* than

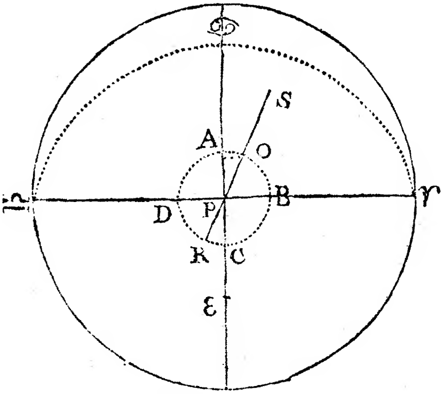
50″ of Preceſſion would cauſe; and it continued to diminiſh till the Year 1736. when the Moon's Aſcending Node was about the Beginning of *Libra*, and her Orbit had the *leaſt* Inclination to the Equator. But by this time, ſome of the Stars near the Solſtitial Colure had altered their Declinations 18″ *leſs*, ſince the Year 1727. than they ought to have done from a Preceſſion of 50″. For γ *Draconis*, which in thoſe nine Years ſhould have gone about 8″ more *Southerly*, was obſerved in 1736. to appear 10″ more *Northerly*, than it did in the Year 1727.

As this Appearance in γ *Draconis*, indicated a Diminution of the Inclination of the Earth's Axis to the Plane of the Ecliptic; and as ſeveral Aſtronomers have ſuppoſed *that* Inclination to diminiſh regularly; if this *Phænomenon* depended upon ſuch a Cauſe, and amounted to 18″ in nine Years, the Obliquity of the Ecliptic would, at that rate, alter a whole Minute in thirty Years; which is much faſter than any Obſervations, *before* made, would allow. I had Reaſon therefore to think, that *ſome Part* of this Motion at the leaſt, if not the *Whole*, was owing to the Moon's Action upon the Equatorial Parts of the Earth; which I conceived, might cauſe a libratory Motion of the Earth's Axis. But as I was unable to judge, from only nine Years Obſervations, whether the Axis would entirely recover the ſame Poſition, that it had in the Year 1727. I found it neceſſary to continue my Obſervations thro' a whole Period of the Moon's Nodes; at the End of which I had the Satisfaction to ſee, that the Stars returned into the ſame Poſitions again; as if there had been no Alteration at all in the Inclination of the Earth's Axis:

which fully convinced me, that I had gueſſed rightly as to the Cauſe of the *Phænomena.* This Circumſtance proves likewiſe, that if there be a gradual Diminution of the Obliquity of the Ecliptic; it does not ariſe only from an Alteration in the Poſition of the Earth's Axis, but rather from ſome Change in the Plane of the Ecliptic itſelf: becauſe the Stars, at the End of the Period of the Moon's Nodes, appeared in the ſame Places, with reſpect to the Equator, as they ought to have done, if the Earth's Axis had retained the ſame Inclination to an invariable Plane.

During the Courſe of my Obſervations, our ingenious Secretary of the *Royal Society,* Mr. *John Machin,* being employed in conſidering the Theory of Gravity; and its Conſequences, with regard to the Celeſtial Motions; I acquainted him with the *Phænomena* that I had obſerved: and at the ſame time mentioned, *what* I ſuſpected to be the Cauſe of them. He ſoon after ſent me a Table, containing the Quantity of the annual Preceſſion in the various Poſitions of the Moon's Nodes, as alſo the correſponding Nutations of the Earth's Axis; which was computed upon the *Suppoſition,* that the *mean* annual Preceſſion is 50″, and that the Whole is governed by the Pole of the Moon's Orbit only: and therefore he imagined, that the Numbers in the Table would be too *large*; as in Fact they were found to be. But it appeared, that the Changes which I had obſerved, both in the annual Preceſſion and Nutation, kept the ſame Law, as to increaſing and decreaſing, with the Numbers of his Table. Thoſe were calculated upon the Suppoſition, that the Pole of the Equator, during a Period of the Moon's Nodes, moved round in the Periphery of a little Circle, whoſe Center was 23° 29′ diſtant from the Pole of the Ecliptic; having itſelf alſo an angular Motion of 50″ in a Year, about the ſame Pole: The North Pole of the Equator was conceived to be in *that* Part of the ſmall Circle, which is fartheſt from the North Pole of the Ecliptic, at the Time when the Moon's Aſcending Node is in the Beginning of *Aries:* and in the oppoſite Point of it, when the ſame Node is in *Libra.*

Such a Hypotheſis will account for an Acceleration and Retardation of the annual Preceſſion; as alſo for a Nutation of the Earth's Axis: And if the Diameter of the little Circle be ſuppoſed equal to 18″; which is the whole Quantity of the Nutation, as collected from my Obſervations of γ *Draconis:* then all the *Phænomena* in the ſeveral Stars which I obſerved, will be very nearly ſolved by it.

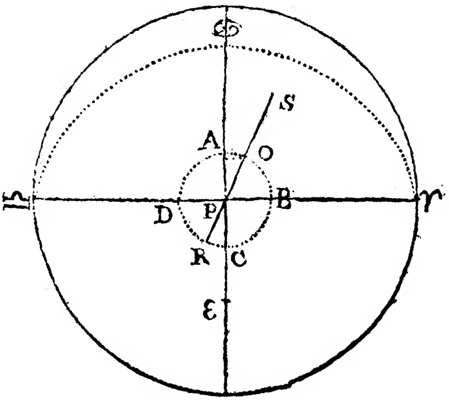


Let *P* repreſent the mean Place of the Pole of the Equator, about which Point, as a Center, ſuppoſe the true Pole to move in the Circle *ABCD*, whoſe Diameter is 18″. Let *E* be the Pole of the Ecliptic, and *EP* be equal to the mean Diſtance between the Poles of the Equator and Ecliptic; and ſuppoſe the true

Pole of the Equator to be at *A*, when the Moon's Aſcending Node is in the Beginning of *Aries*; and at *B*, when the Node gets back to *Capricorn*; and at *C*, when the ſame Node is in *Libra*: at which time the North Pole of the Equator being nearer the North Pole of the Ecliptic, by the whole Diameter of the little Circle *AC* equal to 18″; the Obliquity of the Ecliptic will then be ſo much *leſs* than it was, when the Moon's Aſcending Node was in *Aries*. The Point *P* is ſuppoſed to move round *E*, with an equal retrograde Motion, anſwerable to the mean Preceſſion ariſing from the joint Actions of the Sun and Moon: while the true Pole of the Equator moves round *P*, in the Circumference *ABCD*, with a retrograde Motion likewiſe, in a Period of the Moon's Nodes, or of eighteen Years, and ſeven Months. By this means, when the Moon's Aſcending Node is in *Aries*, and the true Pole of the Equator at *A*, is moving from *A* towards *B*: it will approach the Stars, that come to the Meridian with the Sun about the Vernal Equinox; and recede from thoſe that come with the Sun near the Autumnal Equinox, *faſter* than the *mean* Pole *P* does. So that, while the Moon's Node goes back from *Aries* to *Capricorn*, the *apparent* Preceſſion will ſeem ſo much *greater* than the *mean*; as to cauſe the Stars, that lie in the Equinoctial Colure, to have altered their Declination 9″, in about four Years and eight Months, *more* than the mean Preceſſion would do: and in the ſame time, the North Pole of the Equator will ſeem to have approached the Stars, that come to the Meridian with the Sun at our Winter Solſtice, about 9″; and to have receded as *much* from thoſe, that come with the Sun at the Summer-Solſtice.

Thus

Thus the *Phænomena* before recited are in general conformable to this Hypotheſis. But to be more particular; let *S* be the Place of a Star, *PS* the Circle of Declination paſſing thro' it, repreſenting its Diſtance from the mean Pole, and ♈*PS* its mean Right Aſcenſion. Then if *O* and *R* be the Points, where the Circle of Declination cuts the little Circle *ABCD*; the *true* Pole will be neareſt that Star at *O*, and fartheſt from it at *R*; the whole Difference amounting to 18″, or to the Diameter of the little Circle. As the true Pole of the Equator is ſuppoſed to be at *A*, when the Moon's Aſcending Node is in *Aries*; and at *B*, when that Node gets back to *Capricorn*; and the angular Motion of the true Pole about *P*, is likewiſe ſuppoſed equal to that of the Moon's Node about *E*, or the Pole of the Ecliptic: ſince, in theſe Caſes, the true Pole of the Equator is 90 Degrees before the Moon's Aſcending Node, it muſt be ſo in all others.



When the true Pole is at *A*, it will be at the ſame Diſtance from the Stars that lie in the Eqninoctial Colure, as the mean Pole *P* is; for I neglect at preſent the Caſe of ſuch Stars as are *very* near the Pole of the Equator; and as the true Pole recedes back from *A* towards *B*, it will approach the Stars, that lie in that Part of the Colure repreſented by *P*♈; and recede from thoſe, that lie in *P*♎; not indeed with an *equable* Motion; but in the *Ratio* of the *Sine* of the Diſtance of the Moon's Node from the Beginning of *Aries*. For if the Node be ſuppoſed to have gone backwards from *Aries* 30°, or to the Beginning of *Piſces*; the Point, which repreſents the Place of the true Pole, will in the mean time, have moved in the little Circle, thro' an Arc, as *AO*, of 30° likewiſe: and would therefore in Effect have approached the Stars that lie in the Equinoctial Colure *P*♈, and have receded from thoſe that lie in *P*♎, $4''\frac{1}{2}$; which is the *Sine* of 30° to the *Radius AP*. For if a Perpendicular fall from *O* upon *P A*, it may be conceived as Part of a great Circle, paſſing thro' the true Pole and any Star lying in the Equinoctial Colure. Now the ſame Proportion, that holds in theſe Stars, will obtain likewiſe in all others; and from hence we may collect a general Rule, for finding how much nearer or farther, any particular Star is, to or from, the *mean* Pole, in any given Poſition of the Moon's Node.

For, if *from the Right-Aſcenſion of the Star, we ſubſtract the Diſtance of the Moon's Aſcending Node from Aries; then the Radius will be to the Sine of the Remainder, as 9, is to the Number of Seconds, that the Star is nearer to, or farther from the True, than the Mean Pole.* When that Remainder is *leſs* than 180°, the Star is *nearer* to the True, than to the Mean Pole; and the contrary, when it is *greater* than 180°.

This Motion of the *true* Pole, about the *mean* at *P*, will alſo produce a Change in the Right Aſcenſions of the Stars, and in the Places of the Equinoctial Points; as well as in the Obliquity of the Ecliptic: and the Quantity of the Equations, in either of theſe Caſes, may be eaſily computed for any given Poſition of the Moon's Nodes. But as it may be needleſs, to dwell longer on the Explication of the Hypotheſis; I ſhall now proceed to ſhew its Correſpondency with the *Phænomena*, relating to the Alterations of the Polar Diſtances of ſome of the Stars which I have obſerved: by laying before your Lordſhip the Obſervations themſelves, together with the Computations that are neceſſary; in order to form a right Judgment about the Cauſe of theſe Appearances.

I have endeavoured to find the exact Quantity of the *mean* Preceſſion of the Equinoctial Points, by comparing my own Obſervations made at *Greenwich*, with thoſe of *Tycho Brahè* and others, which I judged to be moſt proper for that Purpoſe. But as many of the Stars, which I compared, gave a different Quantity; I ſhall aſſume the mean Reſult; which gives a Preceſſion of one Degree in ſeventy-one Years and an half: this agreeing very well likewiſe with my Obſervations that were taken at *Wanſted*. The Numbers in the following Tables, which expreſs the Change of Declination in each Star, are computed upon the Suppoſition, that the *mean* Obliquity of the Ecliptic was 23°. 28′. 30″, and that it continued the *ſame*, during the whole Courſe of my Obſervations. And as the Moon's Aſcending Node was in the Beginning of *Aries* about the 27th Day of *March* 1727, I have reduced the Place of each Star to *that* Time; by allowing the proper Change of Declination from that Day, to the Day of each reſpective Obſervation.

It being alſo neceſſary to make an Allowance for the *Aberrations* of Light; I have again examined my Obſervations, that were moſt proper to determine the Tranſverſe Axis of the Ellipſis, which each Star ſeems to deſcribe; and have found it to be neareſt to 40″; which Number I therefore make uſe of in the following Computations.

The Diviſions or Points upon the Limb of my Sector are placed five Minutes of a Degree from each other; and are numbered ſo, as to ſhew the Polar Diſtances nearly; the *true* Polar Diſtance exceeding that, which is ſhewn by the Inſtrument, about 1′. 35″. When I firſt began to obſerve, I generally made uſe of *that* Point on the Limb, which was neareſt to the Star's Polar Diſtance, without regarding whether it was more Northerly, or more Southerly than the Star: but as it ſometimes happened, that the Original Point, with which I at firſt compared the Star, became, in Proceſs of Time, pretty remote from it; I afterwards brought the Plummet to another Point, that was nearer to it; and carefully examined, what Number of Revolutions of the Screw of the Micrometer *&c.* correſponded to the Diſtance between the different Points, that I had made uſe of: by which means I was able to reduce all the Obſervations of the ſame Star to the ſame Point, without ſuppoſing the ſeveral Diviſions to be accurately 5′ aſunder.

I have expreſſed the Diſtance of each Star from the Point of the Arc, with which it was compared, in *Seconds* of a Degree and *tenth Parts* of a Second, exactly as it was collected from the Obſervations; altho' I am ſenſible, that the Obſervations themſelves are liable to an Error of more than a *whole* Second; becauſe I meet with ſome, that have been made within two or three Days of each other, that differ 2″, even when they are not marked as *defective* in any reſpect.

It would be too tedious, to ſet down the whole Number of the Obſervations that I have made; and therefore I ſhall give only enough of them, to ſhew their Correſpondency with the 'forementioned Hypotheſis in the ſeveral Years, wherein any were made of the Stars here recited. When *ſeveral* Obſervations have been taken of the ſame Star, within a few Days of each other; I have either ſet down the mean Reſult, or *that* Obſervation which beſt agreed with it. I have likewiſe commonly choſen thoſe, that were made near the ſame Seaſon of the Year, in ſuch Stars as gave me the Opportunity of making that Choice; particularly in γ *Draconis*, which was generally obſerved about the End of *Auguſt* or the Beginning of *September*; *That* being the uſual Time, when I went to *Wanſted* on purpoſe to obſerve both *that*, and alſo ſome of the Stars in the *great Bear*. But the Weather proving cloudy at that Seaſon in the Year 1744, prevented my making a ſingle Obſervation, either of γ *Draconis*, or any other Star, while I was there; which is the Cauſe of one Vacancy in a Series of 20 ſucceeding Years, wherein that particular Star had been obſerved. Such Stars, as were either not viſible in the Day-time, towards the Beginning of *September*, or came at ſuch Hours of the Night, as would have incommoded the Family of the Houſe wherein the Inſtrument is fixed, were but ſeldom obſerved, after I went to reſide at *Oxford:* which is the Reaſon, why the Series of Obſervations of *thoſe* is ſo imperfect, as ſometimes to leave a Chaſm for ſeveral Years together. But notwithſtanding this, I doubt not, but upon the whole they will be found ſufficient, to

ſatisfy

ſatisfie your Lordſhip of the general Correſpondency between the *Hypotheſis* and the *Phænomena*, in the ſeveral Stars; however different their Situations are, with reſpect to the Cardinal Points of the Equator.

As I made more Obſervations of γ *Draconis* than of any other Star; and it being likewiſe very near the Zenith of *Wanſted*; I will begin with the Recital of ſome of them. The Point upon the Limb, with which this Star was compared, was 38°. 25′ from the North Pole of the Equator, according to the Numbers of the Arc of my *Sector*. The firſt Column, in the following Table, ſhews the Year and the Day of the Month, when the Obſervations were made; the next gives the Number of *Seconds*, that the Star was found to be *South* of 38°. 25′: the third contains the Alterations of the Polar Diſtance, which the *mean* Preceſſion, at the rate of one Degree in 71½ Years, would cauſe in this Star, from the 27th Day of *March* 1727, to the Day on which the Obſervation was taken: the fourth ſhews the Aberrations of Light: the fifth, the Equations ariſing from the 'forementioned Hypotheſis: and the ſixth gives the *mean* Diſtance of the Star from the Point with which it was compared, found, by collecting the ſeveral Numbers, according to their Signs, in the 3d, 4th, and 5th Columns, and applying them to the *Obſerved Diſtances* contain'd in the Second.

If the Obſervations had been perfectly exact, and the ſeveral Equations of their *due* Quantity; then all the Numbers in the laſt Column would have been equal; but ſince they differ a little from one another; if the *mean* of All be taken, and and the Extremes are pared with it, we ſhall find no greater Difference, than what may be ſuppoſed to ariſe from the Uncertainty of the Obſervations themſelves; it no where amounting to more than 1″$\frac{1}{2}$. The Hypotheſis therefore ſeems, in this Star, to agree extremely well with the Obſervations here ſet down; but as I had made above 300 of it; I took the Trouble of comparing each of them with the Hypotheſis: and altho' it might have been expected, that, in ſo large a Number, ſome great Errors would have occurred; yet there are very few, *viz.* only eleven, that differ from the mean of theſe ſo much as 2″; and not one that differs ſo much as 3″. This ſurpriſing Agreement, therefore, in ſo long a Series of Obſervations, taken in all the various Seaſons of the Year, as well as in the different Poſitions of the Moon's Nodes, ſeems to be a ſufficient Proof of the Truth, both of *this* Hypotheſis, and alſo of *that* which I formerly advanced, relating to the Aberrations of Light; ſince the Polar Diſtance in this Star may differ, in certain Circumſtances, almoſt a Minute, *viz.* 56″$\frac{1}{2}$, if the Corrections reſulting from both theſe Hypotheſes are neglected; whereas, when thoſe Equations are rightly applied, the mean Place of the Star comes out the ſame, as nearly, as can be reaſonably expected.

γ *Draconis*		South of 38° 25′	Preceſſion.	Aberration.	Nutation.	Mean Diſt.
		″	″	″	″	″
1727 September	3	70.5	— 0.4	+ 19.2	— 8.9	80.4
1728 March	18	108.7	— 0.8	— 19.0	— 8.6	80.3
September	6	70.2	— 1.2	+ 19.3	— 8.1	80.2
1729 March	6	108.3	— 1.6	— 19.3	— 7.4	80.0
September	8	69.4	— 2.1	+ 19.3	— 6.9	80.2
1730 September	8	68.0	— 2.9	+ 19.3	— 3.4	80.5
1731 September	8	66.0	— 3.8	+ 19.3	— 1.0	80.5
1732 September	6	64.3	— 4.6	+ 19.3	+ 2.0	81.0
1733 Auguſt	29	60.8	— 5.4	+ 19.0	+ 4.8	79.2
1734 Auguſt	11	62.3	— 6.2	+ 16.9	+ 6.9	79.9
1735 September	10	60.0	— 7.1	+ 19.3	+ 7.9	80.1
1736 September	9	59.3	— 8.0	+ 19.3	+ 9.0	79.6
1737 September	6	60.8	— 8.8	+ 19.3	+ 8.5	79.8
1738 September	13	62.0	— 9.6	+ 19.3	+ 7.0	78.7
1739 September	2	66.6	— 10.5	+ 19.2	+ 4.7	80.0
1740 September	5	70.8	— 11.3	+ 19.3	+ 1.9	80.7
1741 September	2	75.4	— 12.1	+ 19.2	— 1.1	81.4
1742 September	5	76.7	— 12.9	+ 19.3	— 4.0	79.1
1743 September	2	81.6	— 13.7	+ 19.1	— 6.4	80.6
1745 September	3	86.3	— 15.4	+ 19.2	— 8.9	81.2
1746 September	17	86.5	— 16.2	+ 19.2	— 8.7	80.8
1747 September	2	86.1	— 17.0	+ 19.2	— 7.6	80.7

I made about 250 Obſervations of β *Draconis*; which I find correſpond as well with the Hypotheſis, as thoſe of γ; but ſince the Poſitions of both theſe Stars, in reſpect to the Solſtitial Colure, differ but little from each other; it will be needleſs to ſet down the Obſervations of β. I ſhall therefore proceed to lay before your Lordſhip, ſome Obſervations of a ſmall Star, that is almoſt oppoſite to γ *Draco-*

nis

conis in Right Aſcenſion, being the 35th *Camelopardali Hevel.* in the *Britiſh* Catalogue. Mr. *Flamſteed*, indeed, has not given the Right Aſcenſion of this Star; but *that* being neceſſary to be known, in order to compute the Change of its Declination ariſing from the Preceſſion of the Equinox; I compared the Time of its Tranſit over the Meridian, with that of ſome other Stars near the ſame Parallel; whereby I found, that its Right Aſcenſion was 85°. 54.′½ at the Beginning of the Year 1737.

This ſmall Star was compared with the ſame Point of the Limb of my Sector, as γ *Draconis*; and the ſecond Column, in the following Table, ſhews how many *Seconds* it was found to be South of that Point, at the time of each reſpective Obſervation. The other Columns contain, as in the foregoing Table, the Equations that are neceſſary to find, what its *mean* Diſtance from the ſame Point would have been on the 27th Day of *March* 1727, which is exhibited in the laſt Column. The whole Number of my Obſervations of this Star did not much exceed forty; the greateſt Part of which were made before the Year 1730; in ſome of the following Years none were taken; and only a ſingle one in any other, except in 1739. However, their Correſpondency ſeems ſufficient to evince the Truth of the Hypotheſis: for if the Mean of theſe, contain'd in the Table, be taken, not one, among the reſt of the Obſervations, will differ from it more than 2″.

35th

35th *Camelopard. Hevelii.*	South of 38° 25′	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South.
	″	″	″	″	″
1727 October 20	73.6	+ 0.9	− 6.7	+ 8.9	76.7
1728 January 12	60.8	1.2	+ 6.1	8.8	76.9
March 1	57.8	1.4	+ 9.4	8.7	77.3
September 26	75.2	2.3	− 8.8	8.1	76.8
1729 February 26	56.4	2.8	+ 9.4	7.6	76.2
1730 March 3	57.8	4.4	9.4	5.4	77.0
1731 February 5	59.1	5.6	8.5	+ 3.0	76.2
1733 January 31	64.1	8.7	8.2	− 2.9	78.1
1738 December 30	61.8	17.2	4.3	6.5	76.8
1739 February 4	56.9	17.3	8.5	6.3	76.4
1740 January 20	56.0	18.6	7.0	− 4.0	77.6
1747 February 27	32.3	28.5	9.4	+ 8.4	78.6

The Obſervations of the forgoing Stars are the moſt proper, to prove the Change of the Inclination of the Earth's Axis to the Plane of the Ecliptic; thoſe, which follow, will ſhew in what manner the Stars, that lie near the Equinoctial Colure, are affected, as well as others, that are differently ſituated, with reſpect to the Cardinal Points of the Equator. Some of theſe Stars are indeed more remote from the Zenith, than I would have choſen, if there had been others, of equal Luſtre, in more proper Poſitions; becauſe Experience has long ſince taught me, that the Obſervations of ſuch Stars, as lie near the Zenith, do generally agree beſt with one another, and are therefore the fitteſt to prove the Truth of any Hypotheſis. I ſhall begin with thoſe near the Vernal Equinox. α *Caſſiopeæ* was compared with the Point marked 34°. 55′; and at firſt was found to be more *Southerly*, but afterwards became more *Northerly* than that Point, as in the following Table; the laſt Column of which ſhews its mean Diſtance *South* of that Point on the 27th of *March* 1727. The Obſervation of the 23d Day of *December*, in the Year 1738, differs 3″ from the *mean* of the others; as does alſo another, that was taken five Days after this; neither of which being marked as uncertain, I judged it proper to inſert one of them; altho' they give the mean Place of the Star near 2 Seconds more *Northerly* than any other, in a Series of above 100; *all* of which correſpond, with the *mean* of theſe here recited, within leſs than 2″; excepting *two*, that give the Stars mean Diſtance almoſt 3″ more *Southerly*; but theſe laſt mentioned are marked as dubious; and indeed they appear to have been bad, by comparing them with ſeveral others, that were made near the ſame time, from which they differ almoſt 2″.

α *Caſſiopeæ.*	South of ° ′ 34. 55	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South
	″	″	″	″	″
1727 September 9	55.0	+ 9.0	+ 2.2	+ 2.4	68.6
1728 September 17	30.8	29.4	+ 4.6	5.2	70.0
1729 June 8	35.7	43.8	— 16.3	6.8	70.0
December 3	N. 9.4	53.5	+ 16.5	7.7	68.3
1730 June 11	S. 13.8	64.0	— 16.2	8.4	70.0
December 9	N. 30.8	73.8	+ 16.3	8.8	68.1
1732 January 8	N. 49.2	95.4	12.9	8.9	68.0
1733 January 21	64.8	116.0	+ 10.0	7.9	69.1
1734 June 13	62.8	143.8	— 6.1	5.0	69.9
December 11	105.4	153.7	+ 16.2	+ 3.7	68.2
1738 December 23	176.3	234.0	+ 15.2	— 7.2	65.7
1740 June 2	169.1	262.8	— 16.5	— 8.9	68.3
1747 February 27	332.3	397.0	+ 0.2	+ 4.7	69.6

Altho' I have taken no Obſervation of *τ Perſei* ſince the 22d Day of *January* 1740; yet, as this Star is very near the Zenith, and a ſufficient Number were made about the Times when the Equation, reſulting from the Hypotheſis, was at its *Maximum*; I judged it proper to inſert ſome of them in the next Table; the laſt Column of which ſhews, how much the Star's *mean* Diſtance was *South* of 38°. 20′. on the 27th Day of *March* 1727. Among near 60 Obſervations I meet with two only, that differ from the mean of theſe ſo much as 2″; and thoſe differ almoſt as much from the mean of others, that were taken near the ſame time: ſo that the Hypotheſis ſeems to correſpond, in general, with the Obſervations of this Star as well, as with either of the foregoing.

τ Perſei.	South of 38° 20′	Preceſſion.	Aberration.	Nutation.	Mean Diſt. South.
	″	″	″	″	″
1727 September 16	60.1	+ 7.4	− 3.2	+ 6.7	71.0
December 29	39.7	11.9	+ 12.9	7.2	71.7
1728 December 21	22.5	27.2	12.8	8.7	71.2
1729 December 2	S. 9.2	42.0	11.5	9.0	71.7
1731 January 3	N. 8.2	59.0	12.8	8.3	71.9
1732 January 8	22.0	74.8	12.7	6.7	72.2
1733 January 21	34.6	91.0	11.7	+ 4.3	72.4
1738 December 23	117.0	183.4	12.8	− 9.0	70.2
1740 January 22	132.5	200.2	11.7	8.6	70.8

After the laſt recited Obſervations, it may perhaps ſeem needleſs to add thoſe of *α Perſei*, which is farther from the Zenith; but however, as this Star lies very nearly at an equal Diſtance from the

Equinoctial and Solſtitial Colures, and the Series of Obſervations of it is ſomewhat more complete, than that of τ *Perſei*; I ſhall inſert one at leaſt, for each Year wherein it has been obſerved; whereby it may appear, that the Hypotheſis ſolves the *Phænomena* of Stars in this Situation, as exactly as in others: for if a *mean* be taken of the Numbers in the laſt Column of the following Table, which expreſſes the *mean* Diſtance of the Star *South* of 41°. 5′. on *March* 27th 1727, it will agree within two Seconds, with every one of 80 Obſervations, that have been made of this Star.

α *Perſei*	South of 41°. 5′.	Preceſſion.	Aberration.	Nutation	Mean Diſt. South.
	″	″	″	″	″
1727 December 29	79.4	+ 10.5	+ 11.4	+ 7.9	109.2
1728 April 7	87.5	14.3	− 0.8	8.2	109.2
July 5	94.6	17.7	− 11.4	8.5	109.4
December 13	65.7	23.8	+ 10.6	8.8	108.9
1729 December 3	53.4	37.2	9.7	8.9	109.2
1731 January 3	38.6	52.3	11.4	7.8	110.1
1732 January 8	26.8	66.2	+ 11.4	+ 5.9	110.3
1734 July 11	S. 21.3	101.0	− 11.4	− 1.1	109.8
1738 December 24	N. 50.3	162.6	+ 11.2	9.0	108.5
1740 January 21	71.8	177.4	10.9	− 8.2	108.3
1747 February 27	182.5	275.4	6.6	+ 8.5	108.0

Having already given Examples of Stars, lying near both the Solſtices and the *Vernal* Equinox; I ſhall now add the Obſervations of *one*, that is not far from the *Autumnal* Equinox, *viz.* η *Urſæ Majoris*, the brighteſt Star in that Part of the Heavens, which approaches the Zenith of *Wanſted* within a Degree;

and which, by reaſon of its Luſtre and Poſition, gave me the Opportunity of making my Series of Obſervations of *It*, more complete than of many others. This Star was compared with the Point marked 39°. 15′. and was *South* of it as in the following Table; wherein your Lordſhip will ſee, that the Obſervations of the Years 1740 and 1741 give the Polar Diſtances 3″ greater, than the *mean* of the other Years. Had there been only a ſingle Obſervation taken in either of thoſe Years, Part of this apparent Difference might have been ſuppoſed to ariſe from their Uncertainty; but as there were 8 Obſervations taken within a Week, either before or after the 3d Day of *June* 1740, which agree well with each other; and three were made within 20 Days in *September* 1741, which likewiſe correſponded with each other; I am inclined to think, that the 'foremention'd Differences muſt be owing to ſomething elſe, beſides the Error of the Obſervations. This *Phænomenon* therefore may deſerve the Conſideration of thoſe Gentlemen, who have employed their Time in making Computations relating to the Quantity of the Effects, which the Power of Gravity may, on various Occaſions, produce. For I ſuſpect, that the Poſition of the Moon's Apogee, as well as of her Nodes, has ſome Relation to the apparent Motions of the Stars that I am now ſpeaking of.

My Series of Obſervations of ſeveral Stars abound, of late Years, with ſo many and long Interruptions; that I cannot pretend to *determine* this Point; but probably the Differences before taken notice of in the Obſervations of α *Caſſiopeæ*, and ſome others that I have found likewiſe among the Obſervations of *other* Stars, that are not here recited, may be owing to ſuch a Cauſe; which, altho' it ſhould not have any large Share of Influence, may yet, in certain Circumſtances, diſcover a Defect in a Hypotheſis, that pays no Regard at all to *It*. But whether theſe Differences do ariſe from the Cauſe already hinted at; or whether they proceed from any Defect of the Hypotheſis itſelf in any other reſpect; it will not be very *material* in point of Practice; ſince *that* Hypotheſis, as it was before laid down, appears to be ſufficient to ſolve all the *Phænomena*, to as great a Degree of Exactneſs, as we can in general *hope* or *expect* to make Obſervations. For if I take the *mean* of all the Numbers in the laſt Column of the following Table for η *Urſæ Majoris*, and compare it with any one of 164 Obſervations that were taken of it, the Difference will not exceed three Seconds.

η *Urſæ Majoris*			South of ° ′ 39. 15	Preceſſion.	Aberration.	Nutation	Mean Diſt. South.
			″	″	″	″	″
1727	October	17	153.3	— 10.2	+ 1.0	— 5.2	138.9
1728	January	24	176.4	15.2	— 17.6	5.8	137.8
	July	17	150.8	23.9	+ 17.8	6.9	137.8
	October	11	170.6	28.2	+ 2.6	7.3	137.7
1729	January	16	196.6	33.1	— 17.8	7.8	137.9
	July	21	170.4	42.4	+ 17.8	8.4	137.4
1730	July	19	189.6	60.6	+ 17.8	9.0	137.8
	December	28	232.4	68.7	— 16.7	8.9	138.1
1731	September	1	218.1	81.9	+ 9.4	8.4	137.2
1732	January	10	250.7	87.7	— 17.7	8.0	137.3
	April	13	238.7	92.3	— 0.8	7.7	137.9
1734	July	1	255.7	133.3	+ 17.6	— 2.3	137.7

η Urſæ Majoris	South of ° ′ 39. 15	Preceſſion.	Aberration.	Nutation	Mean Diſt. South.
	″	″	″	″	″
1735 September 10	280.8	154.6	+ 11.4	+ 1.2	138.8
1736 September 8	294.7	172.8	11.6	4.1	137.6
1737 July 3	303.0	187.8	17.2	6.1	138.5
1738 June 29	319.0	205.8	16.8	7.9	137.9
1739 April 25	348.0	220.8	2.5	8.8	138.5
1740 June 3	360.3	241.1	12.8	8.9	140.9
1741 September 23	390.9	265.0	7.9	+ 7.4	141.2
1745 September 5	466.7	337.1	12.4	− 3.3	138.7
1746 September 20	492.0	356.2	8.8	5.9	138.7
1747 September 2	507.2	373.5	13.2	7.8	139.1

You may perceive, my Lord, by inſpecting the Tables which contain the Obſervations of *α Caſſiopeæ* and *η Urſæ Majoris*; that the greateſt Differences that occur therein may be diminiſhed, by ſuppoſing the *true* Pole of the Equator to move round the Point *P*, in an *Ellipſis*, inſtead of a Circle. For if the tranſverſe Axis, lying in the Direction *AC*, be 18″, and the Conjugate, as *DB*, be about 16″; the Equations, reſulting from ſuch an Hypotheſis, will make the Numbers in the laſt Columns agree with each other, nearer than as they now ſtand. But ſince this would not entirely remove the Inequalities, in all the Poſitions of the Moon's Nodes; I ſhall refer the more accurate Determination of the *Locus* of the *true* Pole to Theory; and at preſent only give the Equations for the Preceſſion of the Equinoctial Points, and the Obliquity of the Ecliptic, as alſo the real Quantity of the annual Preceſſion, to every 5th Degree of the Place of the Moon's Aſcending Node, in the following Tables; juſt as they reſult from the Hypotheſis, as at firſt laid down; it appearing, from what has already been remark'd, that theſe will be ſufficiently exact for Practice in all Caſes.

The Equation of the Equinoct. Points

☽s ☊ from ♈	Sig 0	I	II	Subtr.
	Sig V	VII	VIII	Add
°	″	″	″	°
0	0.0	11.3	19.6	30
5	2.0	13.0	20.5	25
10	3.9	14.5	21.2	20
15	5.8	16.0	21.8	15
20	7.7	17.3	22.2	10
25	9.6	18.5	22.5	5
30	11.3	19.6	22.6	0
Subtr.	Sig. V	IV	III	☽s ☊ from ♈
Add	Sig. XI	X	IX	

The Equation of the Obliquity of the Ecliptick.

☽s ☊ from ♈	Sig. 0	I	II	Add
	Sig. VI	VII	VIII	Subtr.
°	″	″	″	°
0	9.0	7.8	4.5	30
5	9.0	7.4	3.8	25
10	8.9	6.9	3.1	20
15	8.7	6.4	2.3	15
20	8.5	5.8	1.6	10
25	8.2	5.2	0.8	5
30	7.8	4.5	0.0	0
Add	Sig. V	IV	III	☽s ☊ from ♈
Subtr.	Sig. XI	X	IX	

The Annual Preceſſion of the Equinoctial Points.

☽s ☊ from ♈	Sig. 0	I	II	III	IV	V	
°	″	″	″	″	″	″	°
0	58.0	57.0	54.2	50.3	46.5	43.7	30
5	57.9	56.6	53.6	49.7	46.0	43.4	25
10	57.9	56.2	53.0	49.0	45.5	43.2	20
15	57.7	55.7	52.3	48.4	45.0	43.0	15
20	57.5	55.2	51.7	47.7	44.5	42.8	10
25	57.3	54.7	51.0	47.1	44.1	42.8	5
30	57.0	54.2	50.3	46.5	43.7	42.7	0
	Sig. XI	X	IX	VIII	VII	VI	☽s ☊ from ♈

Sir *Iſaac Newton*, in determining the Quantity of the annual Preceſſion from the Theory of Gravity, upon Suppoſition that the Equatorial is to the Polar Diameter of the Earth as 230 is to 229, finds the Sun's Action ſufficient to produce a Preceſſion of 9″$\frac{1}{8}$ only; and, collecting from the Tides the Proportion between the Sun's Force and the Moon's to be as 1 to 4$\frac{1}{2}$, he ſettles the mean Preceſſion, reſulting from their joint Actions, at 50″. But ſince the Difference between the Polar and Equatorial Diameter is found, by the late Obſervations of the Gentlemen of the *Academy of Sciences*, to be greater than what Sir *Iſaac* had computed it to be; the Preceſſion, ariſing from the Sun's Action, muſt likewiſe be greater than what he has ſtated it at, nearly in the ſame Proportion. From whence it will follow, that the Moon's Force muſt bear a leſs Proportion to the Sun's than 4$\frac{1}{2}$ to 1; and perhaps the *Phænomena*, which I have now been giving an Account of, will ſupply the beſt *Data* for ſettling this Matter.

As I apprehend, that the Obſervations already ſet down will be judged ſufficient, to prove in general the Truth of the Hypotheſis before advanced; I ſhall not trouble your Lordſhip with the Recital of more, that I made of Stars lying at greater Diſtances from the Zenith; thoſe not being ſo proper, for the Reaſon before-mention'd, to eſtabliſh the Point that I had chiefly in View. But as it may perhaps be of ſome Uſe to future Aſtronomers, to know what were the *mean* Differences of Declination, at a given Time, between ſome Stars, that lie nearly oppoſite to one another in Right Aſcenſion, and not far from either of the *Colures*; I ſhall ſet down the Reſult of the Compariſon of a few, that differ ſo little in Declination, that I could determine the Quantity of that Difference with great Certainty.

By the *mean* of 64 Obſervations, that were made of α *Caſſiopeæ* before the End of the Year 1728, I collect, after allowing for the Preceſſion, Aberration and Nutation as in the foregoing Tables; that the *mean* Diſtance of this Star was 68″.7 South of 34°. 55′, on the 27th Day of *March* 1727. By a like Compariſon of 40 Obſervations, taken of γ *Urſæ Majoris* during the ſame Interval of Time, I find this Star was, at the ſame time, 39″.6 South of 34°. 45′. I carefully meaſured, with the Screw of the Micrometer, the Diſtance between the Points, with which theſe Stars were compared; and found them to be 9′. 59″ from each other, or one Second leſs than they ought to have been. Hence it follows, that the *mean* Difference of Declination between theſe two Stars, was 10′. 28″.1, on the 27th Day of *March* 1727.

By the *mean* of 65 Obſervations, that were taken of β *Caſſiopeæ*, before the End of the Year 1728, this Star was 25″.8 North of 32°. 20′, on the 27th Day of *March* 1727: and by the *mean* of 52 Obſervations, ε *Urſæ Majoris* was 87″.6 South of 32°. 30′ at the ſame time. The Diſtance between theſe Points was found to be 9′. 59″.3; from whence it follows, that the *mean* Difference of Declination between theſe two Stars was 11′. 52″.7 on *March* 27th 1727.

By the *mean* of 100 Obſervations, taken before the End of the Year 1728, the *mean* Diſtance of γ *Draconis* was 79″.8 South of 38°. 25′ on *March* 27th 1727; and by the *mean* of 35 Obſervations, the 35th *Camelopard. Hevel.* was South of the ſame Spot 76″.4. So that the mean Polar Diſtance of γ *Draconis* was only 3″.4 greater, than that of the 35th *Camelopard. Hevel.* but as the Equation for the Nutation, in both theſe Stars, was then near the *Maximum*, and to be applied with contrary Signs; the *Apparent* Polar Diſtance of γ *Draconis* was 21″.4 greater, on the 27th Day of *March* 1727.

The Differences of the Polar Diſtances of the Stars, as here ſet down, may be preſumed, both on account of the Radius of the Inſtrument and the Number of Obſervations, to be very exactly determined, to the Time when the Moon's Aſcending Node was at the Beginning of *Aries*; and if a like Compariſon be hereafter made, of Obſervations taken of the ſame Stars, near the ſame Poſition of the Moon's Nodes; future Aſtronomers may be enabled, to ſettle the Quantity of the mean Preceſſion of the Equinox, ſo far as it affects the Declination of theſe Stars, with great Certainty: and they may likewiſe diſcover, by means of the Stars near the Solſtitial Colure, from what Cauſe the apparent Change in the Obliquity of the Ecliptic really proceeds, if the mean Obliquity be found to diminiſh gradually.

The 'forementioned Points indeed can be ſettled only on the Suppoſition, that the angular Diſtances of theſe Stars do continue always the ſame, or that they have no real Motion in themſelves; but are at Reſt in Abſolute Space. A Suppoſition, which though uſually made by Aſtronomers, nevertheleſs ſeems to be founded on too uncertain Principles, to be admitted in all Caſes. For if a Judgment may be formed, with Regard to this Matter, from the Reſult of the Compariſon of our beſt modern Obſervations, with ſuch as were formerly made with any tolerable Degree of Exactneſs; there appears to have been a real Change in the Poſition of ſome of the fixed Stars, with reſpect to each other; and ſuch, as ſeems independent of any Motion in our own Syſtem, and can only be referred to ſome Motion in the Stars themſelves. *Arcturus* affords a ſtrong Proof of this: for if its preſent Declination be compared with its Place, as determined either by *Tycho* or *Flamſteed*; the Difference will be found to be much greater, than what can be ſuſpected to ariſe from the Uncertainty of their Obſervations.

It is reaſonable to expect, that other Inſtances of the like kind muſt alſo occur among the great Number of the viſible Stars: becauſe their relative Poſitions may be alter'd by various means. For if our own Solar Syſtem be conceived to change its Place, with reſpect to Abſolute Space; this might, in Proceſs of Time, occaſion an apparent Change in the angular Diſtances of the fixed Stars; and in ſuch a Caſe, the Places of the neareſt Stars being more affected, than of thoſe that are very remote; their relative Poſitions might ſeem to alter; tho' the Stars themſelves were really immoveable. And on the other Hand, if our own Syſtem be at Reſt, and any of the Stars really in Motion, this might likewiſe vary their apparent Poſitions; and the more ſo, the nearer they are to us, or the ſwifter their Motions are, or the more proper the Direction of the Motion is, to be rendered perceptible by us. Since then the Relative Places of the Stars may be changed from ſuch a Variety of Cauſes, conſidering that amazing Diſtance at which it is certain ſome of them are placed, it may require the Obſervations of many Ages, to determine the Laws of the apparent Changes, even of a ſingle Star: much more difficult therefore muſt it be, to ſettle the Laws relating to all the moſt remarkable Stars.

When the Cauſes, which affect the Places of all the Stars in general are known; ſuch as the Preceſſion, Aberration, and Nutation; it may be of ſingular Uſe, to examine nicely the relative Situations of particular Stars: and eſpecially of thoſe of the greateſt Luſtre, which, it may be preſumed lie neareſt to us, and may therefore be ſubject to more ſenſible Changes; either from their own Motion, or from that of our Syſtem. And if at the ſame time that the brighter Stars are compared with each other, we likewiſe determine the relative Poſitions of ſome of the *ſmalleſt* that appear near them, whoſe Places can be aſcertained with ſufficient Exactneſs; we may perhaps be able to judge to what Cauſe the Change, if any be obſervable, is owing. The Uncertainty that we are at preſent under, with reſpect to the Degree of Accuracy wherewith former Aſtronomers could obſerve, makes us unable to determine ſeveral Things, relating to the Subject that I am now ſpeaking of: but the Improvements, which have of late Years been made in the Methods of taking the Places of the heavenly Bodies, are ſo great, that a few Years may hereafter be ſufficient, to ſettle ſome Points; which cannot now be ſettled, by comparing even the earlieſt Obſervations with thoſe of the preſent Age.

It were to be wiſh'd therefore, that ſuch Perſons as are provided with proper Inſtruments, would attempt to determine, with great Care, the preſent relative Poſitions of ſeveral of the Principal Stars, in various Parts of the Heavens; eſpecially of thoſe, that are leaſt affected by Refraction: *that* Cauſe having many times ſo uncertain an Influence on the Places of Objects, that are very remote from the Zenith; that wherever *It* is concerned, the Concluſions, deduced from Obſervations that are *much* affected by it, will always remain doubtful, and too precarious, in many Caſes, to be relied upon.

The Advantages, ariſing from different Perſons attempting to ſettle the ſame Points of Aſtronomy near the ſame time, are ſo much the greater; as a Concurrence in the Reſult, would remove all Suſpicion of Incorrectneſs in the Inſtruments made uſe of. For which Reaſon, I eſteem the curious *Apparatus* at *Shirburn Caſtle*, and the Obſervations there taken, as a moſt valuable *Criterion*, whereby I may judge of the Accuracy of thoſe, that are made at the *Royal Obſervatory*: and as a Lover of Science I cannot but wiſh, that our Nation abounded with more frequent Examples, of Perſons of like Rank and Ability with your Lordſhip, equally deſirous of promoting *This*, as well as every other Branch of Natural Knowledge, that tends to the Honour and Benefit of our Country.

But were the Patrons of Arts and Sciences ever ſo numerous, the Subject of my preſent Letter is of ſuch a Nature, as muſt direct me, to beg Leave to addreſs it to the *Earl of Macclesfield*; not only as a moſt competent Judge of it; but as the *ſole* Perſon, in this Nation, that hath Inſtruments proper to examine into the Truth of the Facts here related. And it is a particular Satisfaction to me, that after ſo long an Attendance upon theſe *Phænomena*, I am allowed the Honour of tranſmitting the Account of them to the Public, thro' your Lordſhip's Hands: as it gives me at the ſame time an Opportunity of profeſſing the grateful Senſe I ſhall ever retain, both of the ſignal Favours which I formerly received from the noble Earl your Father, and of the many recent Obligations conferr'd by yourſelf upon,

My Lord,

Your Lordſhip's

moſt obedient

humble Servant,

Greenwich, Dec. 31.
1747.

Ja. Bradley.